

Interactive comment on “Potential climate forcing of land use and land cover change” by D. S. Ward et al.

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This paper by Ward et al. presents an assessment of the climate forcing induced by past and future land-use and land-cover change.

As far as I know, this paper is the most comprehensive assessment of climate forcing from LULCC, and thus it is worth publishing. I, however, have some questions/suggestions on the scientific aspect of the work, as well as strong concerns as to how the work is presented.

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1 On the methods

1.1 Overall

In this paper, the authors use historical and future land-use and land-cover change data to estimate the RF induced by these activities and to compare it with the one induced by other anthropogenic activities (mainly fossil-fuel burning and industrial activities). Land-cover change, wood harvesting, agriculture and livestock are the anthropogenic land-use-related activities considered in the study. The study then follows the "cause-effect chain" to go from anthropogenic activities to emissions of various compounds, to atmospheric burdens of GHGs and aerosols, and finally to the radiative forcing induced by these compounds.

The overall approach is scientifically sound. It appears to be a reasonable compromise between accuracy and efficiency, though it is subject to several shortcomings that, as long as they are explicitly identified and discussed, do not change the qualitative conclusions of the work.

All these shortcomings and/or inconsistency have to be mentioned in the text (which is not always the case), maybe even in a specific sub-section of the methods section.

1.2 More specific comments

1.2.1

This paper is an attribution exercise, and I think it should clearly be presented as this by using the word "attribution" more often, especially in the abstract, introduction and conclusion.

To this attribution exercise is associated an attribution method called the residual

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method, where the contribution of LULCC is deduced by subtraction of two model realizations: one with all drivers (LULCC + non-LULCC) and one without LULCC (non-LULCC alone). This approach is carried out along the whole study, hence the authors implicitly define the contribution of LULCC as the difference between these two realizations (which raises some issues as to non-linearity, but this is not the point here).

This is at least what I understood. But it does not appear clearly in the text: there should be somewhere (introduction, methods section) a clear statement about this attribution method.

Along with this comment, I am very very uncomfortable with the vocabulary used to discuss this attribution. Especially surrounding table 2, the authors usually refer to the contribution of LULCC as a "change", or they write that LULCC "increased" or "decreased" emissions of some compound in a given year. This is disturbing. All this should be rewritten, using a more dedicated vocabulary like: "contributions from LULCC", "attributed share", "LULCC-induced emissions". Words like "change", "increase" or "decrease" are misleading when comparing two realizations, i.e. two hypothetical worlds, and they should be reserved for temporal dynamics.

1.2.2

There is an opposition (and somehow an inconsistency) between using a complex land model to deduce emissions of some compounds (CO₂, Fire, SOA, Dust), and a simple rescaling for others. I am not asking to redo all the work, but the inconsistency and missing processes should be clearly acknowledged.

For instance, a process-based representation of wetlands (both anthropogenic and natural) could significantly affect the quantitative conclusions of this study about CH₄. Indeed, drying of wetlands in the past has likely decreased CH₄ emissions from wetlands. Also, if rice paddies were taken over natural wetlands, only the difference in

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CH₄ emissions is attributable to the land-use activity (it is not done this way in the RCP). However, little is known about preindustrial wetlands extent, and given it is also affected by climate, it is understandable that it was not included in the study.

Given that CLM includes an explicit N-cycle, I wonder why land-related N-fluxes (i.e. NO_x, NH₃, N₂O) were not taken from the model, while the C-fluxes were. Again, it is alright if the authors are not confident enough to take the fluxes from their model, but it should be clearly stated.

It is unclear if changes in biogenic NMHCs emission induced by LCC (not by deforestation fires, but by changes in PFT fractions and LAI) are also added to anthropogenic emissions from RCP in the calculation of tropospheric O₃ and change in CH₄ lifetime. According to figure 2 it is not, but it could/should be.

1.2.3

This study obviously took some time, and it looks like it begun a while ago, thus results are compared to the AR4. It seems to me that updating the paper with the AR5 would not be too difficult, as it does not require further simulations. Also, the AR5 reference year is 2011, which is closer to the year 2010 used in this study. This would be needed when comparison of results is done (figure 5), and for the rescaling of aerosols effects (which are not so much separated into direct and indirect in the AR5).

By the way, if such an update is feasible in a reasonable amount of time, I also suggest to change the IRF used for CO₂ with the one used in the AR5, chapter 8 (doi:10.5194/acp-13-2793-2013).

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1.2.4

The way CO₂ emissions from LULCC are estimated puzzles me (section 3.2.5). I do not think there should be any downward adjustment! The debate as to what should be included in the CO₂ LULCC flux is still open, and very unlikely to be settled any time soon (see doi:10.5194/esd-4-171-2013 and doi:10.5194/esd-5-177-2014). I'd rather see the authors of this paper choose an attribution method (see above) and stick to it, and not try to correct some biases that only exist for a specific definition of "emissions from land-use change".

Actually, the strongest bias as to CO₂ induced by the way the attribution is done is the inclusion of the loss of potential sink into the CO₂ flux (again, see doi:10.5194/esd-4-171-2013 and doi:10.5194/esd-5-177-2014). But, again, it is only a matter a choice, and I only suggest to state it clearly somewhere in the text, but not to correct it.

The third paragraph of section 3.4.5, discussing the "aerosol BGC effect", actually discusses the carbon-climate feedback. The author decided to complement the IRF approach for CO₂ atmospheric concentration with a simple linear correction to account for this feedback. Although it is a bit crude, I think it is a not-so-bad approach. However, I believe this whole section should be put in the section discussing the atmospheric concentration of CO₂.

2 On the outline

The paper is organized following the conventional introduction-methods-results-conclusion outline. Despite being quite complete, precise and accurate, it is rather tedious to read and sometimes repetitive. I believe the paper could greatly benefit from an overhaul! More specifically, some details should be put in Appendix, to let only the strong message in the main text.

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To me, the main results are: 1) the estimation of the contribution of LULCC to RF in present days and in the future; and 2) the use of CLM to derive some emissions that are not well accounted for in other studies. The creation of a WCS is also of interest given the low likelihood of the RCPs' land-use scenarios. Thence, I would recommend putting in Appendix everything else. There would be 3 main appendixes: the way WCS is created (along with figures 3 and 4), the details of the methods and models used (esp. about atmospheric burden and RF), the uncertainty treatment.

Repetitions could be avoided by organizing the paper per process/phenomenon. Currently, the paper first enumerates all the land-use-related phenomena in introduction, then it describes how they are accounted for in the methods section, and then in the results section the values and limits are discussed. This can lead to double/triple citation of phenomena and/or references that renders the paper heavy.

I recommend an outline like this:

1. Brief introduction (quick context, goal of the study, overview of methods: follow causal-chain from activities to RF).
2. Overview of methods
 - 2.1. LULCC activities (should not include Fires)
 - 2.2. Emissions deduced with CLM (should include Fires, one subsection per compound, should include brief discussion about table 2)
 - 2.3. Emissions not by CLM (one subsection for N₂O, and one for others)
 - 3.4. Radiative forcing (one paragraph for GHGs, one for short-lived species, one for albedo effects; give details in Appendix)
4. Results
 - 4.1. Individual contributions from compound/process (present-day only)
 - 4.2. Overall contribution from LULCC (present-day, RCPs, WCS)
 - 4.3. Enhancement factor
5. Conclusions

With a clear reorganization things like table 1 or introductive paragraph of section 3

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could be removed.

3 Specific points

p.12170 l.4–20 To complete, one could add the change in VOCs emissions due to LCC (e.g. doi:10.1029/2005GL024164), and a reference to Arneeth et al. (doi:10.1038/ngeo905) could be added as they give a comprehensive view of the feedbacks involving the land biosphere.

p.12172 l.24–25 This is no longer true (yet not totally false) as for some compounds the AR5 gives the partition between fossil-fuel and land-use.

p.12177 l.14 Unclear what happens between 2005 and 2010, as RCPs are not defined over this period.

p.12178 l.4 Section 3.1.1. includes a description of environmental changes (CO₂, climate) used with CLM. These are not stricto sensu "LULCC activities". They could be put in Appendix with the details of how CLM is used. By the way, I did not understand how the two different climate projections were used to assess uncertainties...

p.12178 l.16–26 This whole paragraph goes with the way WCS is created (in Appendix).

p.12180 l.9 These are not explicit land-use activities in the study: only implicit as emission data are taken from the RCPs.

p.12181 l.5 Give the precision that N₂O is treated separately because sectoral info is not available from RCPs.

p.12182 l.9 "CAM" acronym is used without being defined.

p.12185 l.14 Davidson (doi:10.1038/ngeo608) did some things about N₂O emissions

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by tropical forest soils. Although uncertainties are not assessed and are certainly high, it is at least an estimate. But this relate to my major comment 1.2.2.

p.12188 l.5 Again, this is an old IRF.

p.12196 l.8–13 I would not mention at all the "uncertainty in policies": it is a very different and peculiar kind of uncertainty, not directly comparable with scientific uncertainty.

p.12199 l.14 I think there is a bias in estimating P and Fe deposition only from fires. More specifically, fossil-fuel burning also emits phosphorus, and I think continental dusts do bear iron as well. Actually, the uncertainty is so high in that field that I would suggest not to account for these two effects at all.

p.12202 l.1 The discussion relative to the enhancement factor should be extended, especially regarding the effect of accounting for uncommon land-use-related emissions (Dust, Fires, etc.). Would another model give significantly different enhancement factors? Are there still missing processes/compounds that could change the results, in one way or another?

table 2 I suggest to add CO₂ emissions (maybe cumulative) to this table. Also, see major comment 1.2.1. about vocabulary and the fact that it should be clear that the emissions presented here are the emissions attributed to LULCC following the chosen method.

table 5 Define AOD.

table 6 Given the importance – in the main message – of the enhancement factor, and given the authors assessed the uncertainty in the study, I strongly suggest to show uncertainty ranges of the factor in this table.

figures The figures are nice. But again, figures 3 and 4 could be put in Appendix.

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